

Improving Deep Learning for Defect Prediction (using the GHOST Hyperparameter Optimizer)

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Abstract—There has been much recent interest in the application of deep learning neural networks in software engineering. Some researchers are worried that deep learning is being applied with insufficient critical assessment. Hence, for one well-studied software analytics task (defect prediction), this paper compares deep learning versus prior-state-of-the-art results. Deep learning will outperform those prior results, but only after adjusting its hyperparameters using GHOST (Goal-oriented Hyperparameter Optimization for Scalable Training). For defect prediction, GHOST terminates in just a few minutes and scales to larger data sets; i.e. it is practical to tune deep learning tuning for defect prediction. Hence this paper recommends deep learning for defect prediction, but only after adjusting its goal predicates and tuning its hyperparameters (using some hyperparameter optimization tool, like GHOST).

Index Terms—defect prediction, hyperparameter optimization, neural networks

1 INTRODUCTION

There has been much recent interest in the application of deep learning (DL) neural networks to (e.g.) bug localization [1], sentiment analysis [2], [3], API mining [4]–[6], effort estimation for agile development [7], code similarity detection [8], code clone detection [9], etc. Some researchers are worried that DL is being applied with insufficient critical assessment. For example, Gary Marcus [10] warns that DL may be so over-hyped that it runs “fresh risk for seriously dashed expectations” that could blinker AI researchers from trying new ideas as well as bringing another AI winter (the period in the mid to late 1980s when most AI companies went bankrupt due to poor generalization of their methods).

So what is the benefit of DL? Is it technology over-hyped? Should we stop using everything else and just move to DL? Very few SE researchers are studying this question. In their literature review, Li et al. [11] explored over 40 SE papers facilitated by DL models and found that 33 of them used DL without comparison to other (non-DL) techniques. In our own literature review on DL in SE (reported below in §2), we find experimental comparisons of DL-vs-non-DL in less than 10% of papers.

Therefore, in order to better understand the benefits of DL, this paper compares deep learning with established results in software analytics; specifically: *defect prediction from static code attributes*. For that comparison, we use a prior state-of-the-art result: specifically, the DODGE hyperparameter optimizer [12]. DODGE is a useful comparison method since it is a recent state-of-the-art result (published in TSE’19). Also, internally, DODGE explores billions of different learners and their configurations. Hence “comparing against DODGE” really means comparing against a very wide range of alternatives. Using defect prediction as a

case study and DODGE as a comparison tool, we ask the following questions.

RQ1: Does standard deep learning work for defect prediction?

For defect prediction, standard deep learners do not perform better than existing state-of-the-art methods in 30/40 experiments.

The key phrase in the last paragraph is “standard deep learners”. For **RQ1**, we ran the deep learners using the standard configurations recommended in the literature. What we show in this paper is that standard deep learning can be greatly improved by first asking:

RQ2: Why does standard deep learning fail for defect prediction?

We find that issues with how the decision boundary is explored can explain the above. Specifically:

The lack-of-success of deep learning in defect prediction can be attributed to optimizing for the wrong performance measure.

This result led to a new tool called GHOST (Goal-oriented Hyperparameter Optimization for Scalable Training). This new tool (a) uses the same “ ϵ -domination” heuristic as DODGE (explained in §2.3.1) while, at the same time, (b) extends DODGE to add a novel “weighted loss function” (explained in §2.3.3). To assess GHOST, we ask:

RQ3: How might we improve the results of deep learners on defect prediction?

Using GHOST, we tune the DL loss function to find:

For most evaluation goals, our modified deep learner performs better than the prior state-of-the-art.

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RQ4: Does deep learning work well in all cases?

For this question, we evaluate deep learners over multiple metrics (area under the ROC curve (AUC), recall, false positive rate, and popt20). GHOST achieves superior results according to 3 of 4 criteria (AUC, recall, and popt20; see Table 7), but according to the fourth criteria (false positive rate), deep learning is defeated by the prior-state-of-the-art. Hence we say:

We recommend the use of deep learning in domains where false alarms are not particularly catastrophic.

Another way to state the conclusion of **RQ4** is that developers need to experiment before selecting the methods(s) that work best in the local domain. In theory, it is hard to conduct these experiments since many researchers report that deep learners have very long training times [13], [14] (e.g. Lee et al. [13] report that training took 23 days on 8 NVIDIA Tesla V100 GPUs). Note that if it is so hard to experiment with deep learning, then it would be hard to apply the lessons of **RQ4**. Accordingly, we must check:

RQ5: How slow is tuning for deep learning for defect prediction?

We find that for the defect prediction data sets studied here, the median training times for deep learners with GHOST's hyperparameter optimization is 10 minutes (median). Also, we find that the tuning time increases at a slower rate than the number of rows in the datasets. e.g. with a 10x increase in the size of the data, the largest increase in the runtime was less than 4x. Hence we say:

For defect prediction, deep learners are both practical and tractable.

In summary, our key research contributions are:

- We show that for defect prediction, off-the-shelf deep learning is not recommended (see **RQ1**).
- We show that, contrary to prior pessimism, tuning deep learning algorithms is both useful (see **RQ2,RQ3**), practical, and tractable (see **RQ5**).
- For many goals, deep learning can out-perform prior state-of-the-art results in defect prediction.
- That's said, deep learning is not recommended for defect prediction for certain goals (see **RQ4**).

We also offer two systems-level contributions:

- **GHOST**, a novel tuning method;
- A reproduction package containing all the code and data used in this study¹.

The rest of this paper is structured as follows. Section 2 offers some background notes. Section 3 discusses our approach in detail, along with our experimental design. Section 4 offers answers to our research questions. In Section 5, we discuss some threats to validity. Our conclusion, in Section 6, will be

For SE, do not use off-the-shelf deep learning. Instead, tune that algorithm to the needs of SE (using tools like, e.g. GHOST).

2 BACKGROUND

2.1 Why study defect prediction?

The case studies of this paper relate to defect prediction. This section reviews why that is worthy of study.

During software development, the testing process often has some resource limitations. For example, the effort associated with coordinated human effort across large code bases can grow exponentially with the scale of the project [26]. Since every quality assurance decision is associated with a human and resource cost to the developer team, it is impractical and inefficient to distribute equal effort to every component in a software system [27]. Hence, it is common to match the quality assurance (QA) effort to the perceived criticality and bugginess of the code for managing resources efficiently. Creating defect prediction models is an efficient way to take a look at the incoming changes and focus on specific modules based on suggestions from a defect predictor.

Such predictors can save labor compared with traditional manual methods. Misirli et al. [28] built a defect prediction model for a telecommunications company. Their models predicted 87% of code defects and decreased inspection efforts by 72% (and reduced post-release defects by 44%). Also, Kim et al. [29] applied the REMI defect prediction mode to API development process at Samsung. Their models could predict the bug-prone APIs with reasonable accuracy (0.68 F1 scores) and reduce the resources required for executing test cases.

Not only that, but defect predictors are also competitive with certain automatic methods. Rahman et al. [30] compared (a) static code analysis tools FindBugs, Jlint, and PMD with (b) defect predictors (which they called "statistical defect prediction") built using logistic regression. No significant differences in cost-effectiveness were observed.

Consequently, there is much interest in the industrial community about defect prediction. In a survey of 395 practitioners from 33 countries and five continents, Wan et al. [31] found that over 90% of the respondents were willing to adopt defect prediction techniques.

2.2 Deep learning for Software Engineering

The rest of this paper explores defect prediction using DL and non-DL methods.

To understand the current state of deep learning in software engineering, in May 2020, we explored the literature as follows. Using Google Scholar, we search for research papers using the keyword "deep learning AND software engineering". This returned over 900 papers with at least one citation. To narrow down that list, we looked for papers published in the top venues (defined as per Google Scholar metrics "software systems"), with $N \geq 10$ cites per year (and for papers more recent than 2017 we used $N \geq 1$). This led to the list of papers in Table 1.

From that survey, it was clear that there is a growing research interest in the use of deep learning for software engineering. For example:

- Zhang et al. [25] model code using a language model after standard Natural Language Processing (NLP) preprocessing steps (tokenization, comment removal, rare word replacement, etc.).

1. See <https://tiny.cc/ghost-dl>.

TABLE 1: Selected papers after applying filters (top SE venues, at least 10 cites per year).

Year	Venue	Cites	Use Cases	Title
2016	ICSE	262	defect prediction	Automatically Learning Semantic Features for Defect Prediction [15]
2016	ASE	224	code clone detection	Deep learning code fragments for code clone detection [9]
2015	MSR	183	code representations	Toward Deep Learning Software Repositories [16]
2017	ICSE	83	trace links	Semantically Enhanced Software Traceability Using Deep Learning Technique [3]
2017	ICSME	58	code clone detection	CCLearner: A Deep Learning-Based Clone Detection Approach [17]
2019	TSE	37	story point estimation	A Deep Learning Model for Estimating Story Points [7]
2018	MSR	34	code clone detection	Deep Learning Similarities from Different Representations of Source Code [10]
2017	ICSME	33	vulnerability prediction	Learning to Predict Severity of Software Vulnerability Using Only Vulnerability Description [18]
2018	TSE	24	defect prediction	Deep Semantic Feature Learning for Software Defect Prediction [19]
2017	ICSME	23	duplicate bug retrieval	Towards Accurate Duplicate Bug Retrieval Using Deep Learning Techniques [16]
2019	ICSE	10	bug localization	CRADLE: Cross-Backend Validation to Detect and Localize Bugs in Deep Learning Libraries [20]
2019	MSR	8	defect prediction	DeepJIT: An End-to-End Deep Learning Framework for Just-in-Time Defect Prediction [21]
2019	MSR	5	defect prediction	Lessons Learned from Using a Deep Tree-Based Model for Software Defect Prediction in Practice [22]
2019	ICSE-NIER	4	transfer learning	Leveraging Small Software Engineering Data Sets with Pre-Trained Neural Networks [23]
2018	TSE	4	defect prediction	How Well Do Change Sequences Predict Defects? Sequence Learning from Software Changes [24]
2018	ICSESS	1	language model	A Neural Language Model with a Modified Attention Mechanism for Software Code [25]

- Wang et al. [15], [19] use a Deep Belief Network (DBN) to learn “semantic” features and then use classical learners to perform defect prediction.
- Pham et al. [20] propose CRADLE, which uses pre-trained language models for bug localization.
- Wen et al. [24] extract semantic feature vectors, discretized, then use them for defect prediction.
- Hoang et al. [21] propose DeepJIT, a deep learning framework for just-in-time defect prediction.
- Li et al. [17] obtain frequency count vectors from source code, learn pairwise similarity scores, and use a vanilla feedforward neural network to find code clones.
- Deshmukh et al. [16] use a Siamese LSTM [32] and a convolutional neural network (CNN) to retrieve duplicate bugs.
- Robbes et al. [23] show proof that deep learners can learn from small datasets by leveraging pre-trained models and ULMFit [33].
- Dam et al. [22] develop a Tree-LSTM [34] to extract a feature vector from the Abstract Syntax Tree of the code (AST) and use it for defect prediction.
- White et al. [9] use recurrent neural networks to extract embeddings for code clone detection. White et al. [35] learn code representations using deep learners.
- Han et al. [18] feed GloVe [36] embeddings to a CNN to extract features, and finally use an SVM classifier for vulnerability prediction.

One feature we note from our sample of research papers is that the code and data for most of these papers is not always open source. Some are even protected by patent applications. This has implications for reproducibility. For example, this paper baselines some of our results against Wang et al. [15]. Their methods are protected by a patent, and therefore we could not replicate their results using their code. That said, we were able to find their exact training and test sets, which we used for comparison purposes.

Figure 1 summarizes key features of that literature. Note that few papers compare their results with non-deep learning methods [15], [22]. Also, Figure 1 shows only one prior result which included deep learning, hyperparameter optimization, and a comparison to non-DL methods [15]. Further, Several of the papers warn of the extreme cost of

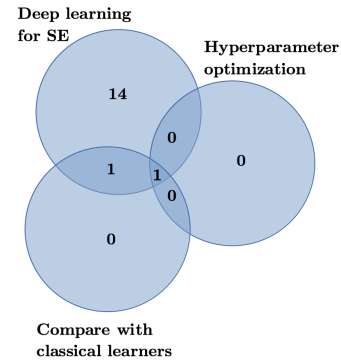


Fig. 1: Summary of Table 1. The paper satisfying all three criteria is [15]; the paper that compares with classical learners is [22]

performing deep learning; specifically, very long training times (e.g. when learning semantic features [15]). Furthermore, most of the papers do not perform a structured hyperparameter optimization (we only see this done by [15], using a grid search approach). Even then, very few hyperparameters are tested.

This lack of hyperparameter optimization in DL defect prediction papers [15], [19], [22] is of some concern. Hyperparameter optimization is the tuning of so-called “hyperparameters”—parameters that are not learned by the algorithm, but instead drive the working of the algorithm—to attain optimal performance. Hyperparameter optimization is very useful when using complex learners. This is especially true for deep learners, which have exponentially more parameters and complex loss surfaces to explore. Montufar et al. [37] note that the expressive power of deep learners comes from the architecture, which determines bounds on the number of piecewise linear regions that can be represented as a decisions.

2.3 Hyperparameter Optimization

Hyperparameter optimization is the automatic tuning of many parameters that control the internal settings of the learner and data preprocessors. The last 15 years have seen a dramatic increase in the application of machine learning and hyperparameter optimization (HPO) to software analyt-

Learners used by DODGE:

- DecisionTreeClassifier(criterion=b, splitter=c, min_samples_split=a). a, b, c = randuniform(0.0,1.0), randchoice(['gini', 'entropy']), randchoice(['best', 'random']).
- RandomForestClassifier(n_estimators=a, criterion=b, min_samples_split=c). a,b,c = randint(50, 150), randchoice(['gini', 'entropy']), randuniform(0.0, 1.0)
- LogisticRegression(penalty=a, tol=b, C=float(c)). a,b,c=randchoice(['l1', 'l2']), randuniform(0.0,0.1), randint(1,500)
- MultinomialNB(alpha=a) = randuniform(0.0,0.1)
- KNeighborsClassifier(n_neighbors=a, weights=b, p=d, metric=c). a, b,c = randint(2, 25), randchoice(['uniform', 'distance']), randchoice(['minkowski', 'chebyshev']). If c=='minkowski': d= randint(1,15) else: d=2

Pre-processors used by DODGE (and the ones in italics were also used by GHOST):

- *StandardScaler*
- *MinMaxScaler*
- *Normalizer(norm=a) = randchoice(['l1', 'l2', 'max'])*
- *MaxAbsScaler*
- RobustScaler(quantile_range=(a, b)) = randint(0,50), randint(51,100)
- *KernelCenterer*
- QuantileTransformer(n_quantiles=a, output_distribution=c, subsample=b). a, b = randint(100, 1000), randint(1000, 1e5); c=randchoice(['normal', 'uniform']).
- Binarizer(threshold=a) = randuniform(0,100)

TABLE 2: Hyperparameter options explored by DODGE [12]. GHOST optimizers uses some of these parameters (see the pre-processing options shown in *italics*) in the manner discussed in Table 3.

ics [38]–[45]. A repeated results is that research results from an “off-the-shelf” learner might be overturned by a second study that tunes the hyperparameters of that learner [12]. For example, in 2008, Lessmann et al. [46] argued that for defect prediction, the best and worst learning methods were random forests and CART, respectively. In 2016, Fu et al. [26] showed that hyperparameter optimization effectively reverses that conclusion since after tuning, CART outperformed random forests.

While automatically tuning various hyperparameters is rare in the SE deep learning literature (see Figure 1), it has been explored in other domains. For example, “AutoML” methods seek the best combination of preprocessors and hyperparameters for a given dataset. These are typically based on Bayesian optimization, as in [47], [48]. However, while Bayesian optimization in practice does find an optimal set of hyperparameters for deep learners, it takes a long time to run. For example, Feurer et al. [47] report Auto-Sklearn results after 48 hours of running on a CPU farm.

Yet another branch, called Neural Architecture Search, also typically uses Bayesian optimization. These techniques aim to find the optimal learner architecture. These techniques have achieved a rather high level of sophistication: for example, Liu et al. [49] describe a hierarchical approach for building neural network architectures. Elsken et al. [50] provide a review of neural architecture search methods. Some of these neural architecture search papers inevitably overlap with hyperparameter tuning [51]–[55].

Both these approaches share the same concerns (a) they have long runtimes (b) some neural architecture search approaches tend to generate overly complex architectures for a problem, which may be overkill for software engineering. Accordingly, the rest of this paper discusses experiments with DL hyperparameter optimization via our GHOST tool. We prefer GHOST over other methods since it is simple to implement, it runs quickly, and it scales to larger data sets.

Table 2 and Table 3 lists hyperparameters that can control the algorithms that generate defect predictors. Table 2, these options were collated from the hyperparameters explored in recent SE papers [26], [56]–[58] and in the documentation of

a widely-used data mining library (Scikit-learn [59]).

As to Table 3, these options come from recent papers on deep learning. After reading the literature looking for a “standard DL architecture”, we use standard feed-forward neural networks with ReLU activation functions (rectified linear units; a.k.a. “hockey sticks”) in the hidden layers and sigmoid activation in the last layer. As to other details of our deep learners, Montufar et al. [37] discuss feed forward neural networks with ReLU activations. The bounds for the number of piecewise linear regions constituting a decision boundary that can be represented by a deep learner, providing a proof based on topological folding. After, Montufar et al., we use no more than 3 layers with up to 20 units.

One options within deep learning is how many “epochs” to apply (and one epoch is one run over all the data to adjust entwork weights). For our experiments, we used ten epochs since, as shown in Figure 2, we found very little improvement after after five epochs (aside: and in experiments with up to 256 epochs, we found little improvements over the results shown in Figure 2).

2.3.1 Optimization with DODGE

One key point to observe from those tables is the size of optimization space. If each numeric range is divided into

TABLE 3: Tuning parameters for DL (used by GHOST)

Preprocessor	Description	Options
<i>StandardScaler</i>	Transforms the dataset to have a mean 0 and variance 1	None
<i>MinMaxScaler</i>	Squeezes the data to the range [0, 1].	None
<i>Normalizer</i>	Normalizes samples to a unit norm.	Choice of norm: l_1 , l_2 , l_∞
<i>NumLayers</i>		1 .. 5
<i>UnitsPerLayer</i>		2 .. 20
<i>Epochs</i>	number of times data reapplied to the algorithm	10 .. 30
<i>Network</i>	topology	Feed forward
<i>Activation</i>	hidden units: last layer	rectified linear units sigmoid

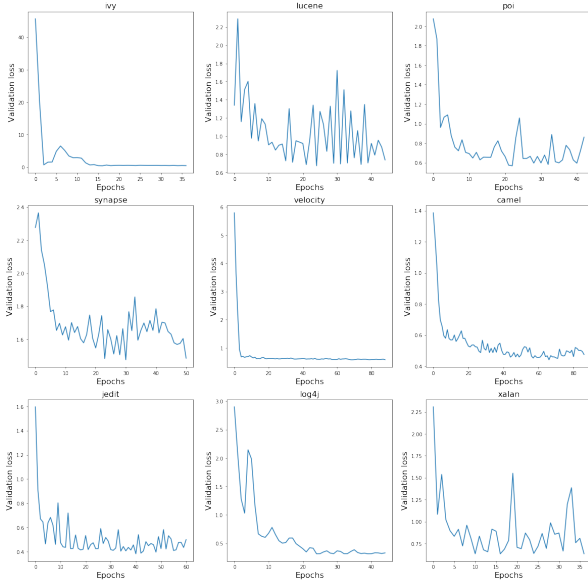


Fig. 2: DL convergence on defect prediction data sets

five bins using $(max-min)/5$, then Table 2 holds nine binary choices and 18 options with five choices; i.e.

$$5^{18} * 2^9 \approx 2 * 10^{15} = 2,000,000,000,000,000 \text{ options}$$

This space is too large to be exhaustively explored. Hence GHOST and DODGE [12] explore this space heuristically. DODGE’s exploration is defined around a concept called ϵ -domination. Proposed by Deb in 2005 [60], ϵ -domination states that there is little point in optimizing two options if their performance differs by less than some small value ϵ .

DODGE exploits ϵ -domination as follows. N times do: (a) generate configurations at random, favoring those those with lowest weight (initially, all options have a weight $w = 1$); (b) if one option generates results within ϵ of some other, then increases these options’ weights (thus decreasing the odds that they will be explored again). This approach can be a very effective way to explore a large space of options. If we assess a defect predictor on $d = 2$ dimensions (e.g. recall and false alarm), the output space of that performance divides into $(1/\epsilon)^d$ regions.

To understand the practical implications of this, we note that defect predictors can exhibit a large variance in their output, particularly for data sets like those in Table 5 (where the size of the target class in the training and test data is so variable). Assuming $d = 2$ and $\epsilon = 0.2$ then our defect predictors have $(1/0.2)^2 = 25$ output regions. Each time we apply DODGE’s tabu search, then two of these regions can be removed; i.e. in theory, DODGE could terminate very quickly.

Agrawal et al. [12] found that, for defect prediction, the optimizations found above $N = 30$ repeats of the above loop performed no worse than those found after $N = 1000$ loops. Better yet, the optimizations found that way outperformed numerous prior results.

2.3.2 Optimization with GHOST

The lesson of DODGE is that the particulars of Table 2 can be less important than how we explore combinations of

learning methods. So can we do more with that insight? Is there some process more direct than DODGE that explores, in a more direct way, how to divide data into regions that (e.g.) do or do not predict for defects?

This section proposes GHOST as an answer to those questions. To understand GHOST, we first take a glance at our defect prediction data. Table 4 shows the static code attributes seen in our data and Table 5 shows what releases we use for our training and test sets when comparing with DODGE² (when comparing against our other baseline by Wang et al. [19], we use the same train and test release versions as they do). A curious feature of Table 5 is the wide variability in the percent of defects seen in the training and test sets. For example, the defect ratios in:

- velocity’s train:test ratios decrease from 71 to 34%;
- jEdit’s train:test sets ratios decrease from 23 to 2%;
- xerces’ train:test sets ratios increase from 16 to 74%.
- log4j’s train:test sets ratios increase from 29 to 92%;
- xalan’s train:test sets ratios increase from 38 to 99%;

Such massive changes in the target class ratios means that the geometry of the hyperspace boundary between different classifications can alter dramatically between train and test. Therefore, the “appropriate learner” for this kind of data is one that works well for a wide range of ratios of class distributions. Such an appropriate learner knows how to exploit “hints” from the hyperspace boundary in the train set, then apply those hints to the test set.

GHOST was implemented as an experiment to see if the *loss functions* of neural networks can learn such “hints”. What the rest of this paper shows is that such loss function (plus some fuzzy oversampling, described below) is an effective method for handling defect prediction.

2.3.3 Loss Functions

In neural networks, the loss function is the prediction error seen in the last epoch of the neural net. The loss is used to calculate the gradients used to update the weights of the neural net for the next epoch of learning. Loss can be calculated in various ways³ but the key point to note here is that, usually, the loss function is fixed prior to learning.

In 2019, Cui et al. [66] proposed dynamically adjusted loss functions that know how to adjust the loss function in order to reduce the overlap between sets of positive and negative examples. Their re-weighting scheme uses the effective number of samples for each class to re-balance the loss, thereby yielding a class-balanced loss. There experiments show that, when trained with a dynamic class-balanced loss function, the network is able to achieve significant performance gains on data sets with class imbalance.

The methods of Cui et al. assumed high dimensional vision data (with, sometimes, over 8000 classes), so we can not directly apply their methods here. However, inspired by their work, we did design a class balance method that proved to be effective for defect prediction. To illustrate that method, we artificially generated two sets of data with very

2. These releases were selected as follows: in the available data, use recent releases to predict for defects in the latest release.

3. mean squared error, binary crossentropy, categorical crossentropy, sparse categorical crossentropy, etc.

TABLE 4: Static attributes in our datasets.

Attribute	Description (with respect to a class)
wmc	Weighted methods per class [61]
dit	Depth of inheritance tree [61]
noc	Number of children [61]
cbo	Coupling between objects [61]
rfc	Response for a class [61]
lcom	Lack of cohesion in methods [61]
lcom3	Another lcom metric proposed by Henderson-Sellers [62]
npm	Number of public methods [63]
loc	Number of lines of code [63]
dam	Fraction of private or protected attributes [63]
moa	Number of fields that are user-defined types [63]
mfa	Fraction of accessible methods that are inherited [63]
cam	Cohesion among methods of a class based on parameter list [63]
ic	Inheritance coupling [64]
cbm	Coupling between methods [64]
amc	Average method complexity [64]
ca	Number of classes depending on a class [64]
ce	Number of classes a class depends on [64]
max_cc	Maximum McCabe’s cyclomatic complexity score of methods [65]
avg_cc	Mean of McCabe’s cyclomatic complexity score of methods [65]

TABLE 5: Evaluated software projects for comparing with DODGE [12]

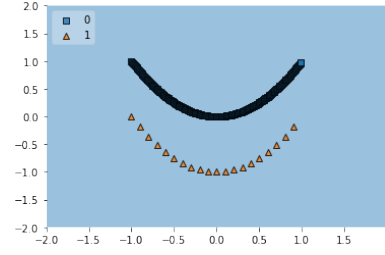
Project	Train versions	Test versions	Training Buggy %	Test Buggy %
ivy	1.1, 1.4	2.0	22	11
lucene	2.0, 2.2	2.4	53	60
poi	1.5, 2.0, 2.5	3.0	46	65
synapse	1.0, 1.1	1.2	20	34
velocity	1.4, 1.5	1.6	71	34
camel	1.0, 1.2, 1.4	1.6	21	19
jEdit	3.2, 4.0, 4.1, 4.2	4.3	23	2
log4j	1.0, 1.1	1.2	29	92
xalan	2.4, 2.5, 2.6	2.7	38	99
xerces	1.2, 1.3	1.4	16	74

different class distributions. This data had 200 rows and three columns:

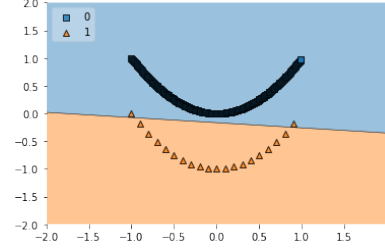
- A class column, whose values are selected at random, with the negative class being 10 times more likely than the positive class)
- Two other columns which we call x_1, x_2 where x_1 is selected randomly -1 to 1 and x_2 is
 - For the negative examples, $x_2 = x_1^2$;
 - For the positive examples $x_2 = x_1^2 - 1$.

We then ran a deep learner (with one hidden layer) for 10 epochs, with and without the weighted loss function (described below). Figures 3a and 3b show the results. Observe how, with an unweighted loss function, the neural network completely ignores the positive class, but with the weighted version, it recognizes the importance of the positive examples, pushing the accuracy up from 90.91% to 99.09%.

We now elaborate on the weighted loss functions used in Figure 3b. Consider a loss function $\mathcal{L}(y, \hat{y})$ (where y is a vector representing the target outputs, and \hat{y} is a vector representing the predictions) optimized by a neural network.



(a) Unweighted loss function



(b) Weighted loss function

Fig. 3: The two arcs in these figures show points selected from two distributions: $y_1 = x_1^2$ and $y_2 = x_1^2 - 1$ (where y_1 is ten times more frequent than y_2). The background colors show the hyperspaces learned by a neural network. Note that, in the top plot (using an unweighted loss function), the hyperspace is all one color; i.e, it does not distinguish between the two populations. But in the lower plot, the hyperspace divided into blue and orange, where orange denotes the region where the neural net predicts that the data comes from the minority $x_1^2 - 1$. distribution. Observe how the unweighted loss function misses all the minority class with while the weighted loss function can find nearly all of the minority class.

As is the case with defect prediction, consider the case of binary classification. Let c_0 be the minority class, and C be the set of all classes; for binary classification, this is simply the set $\{c_0, c_1\}$. Denote a training example by a subscript (i.e., let y_i denote the target of the i th training example), and let m be the number of training examples. Then, the loss function can be rewritten as:

$$\mathcal{L}(y, \hat{y}) = \sum_{i=1}^m \mathcal{L}(y_i, \hat{y}_i) = \sum_{\substack{i=1 \\ y_i=c_0}}^m \mathcal{L}(y_i, \hat{y}_i) + \sum_{\substack{i=1 \\ y_i \neq c_0}}^m \mathcal{L}(y_i, \hat{y}_i)$$

We call the above the *unweighted* loss function. Now, we apply a fairly obvious method of weighting the above as follows. Let n be the fraction of samples with class c_0 . Then, we re-weight the loss function above to:

$$\mathcal{L}^1(y_i, \hat{y}_i) = \frac{w_i}{n} \sum_{\substack{i=1 \\ y_i=c_0}}^m \mathcal{L}(y_i, \hat{y}_i) + \sum_{\substack{i=1 \\ y_i \neq c_0}}^m \mathcal{L}(y_i, \hat{y}_i) \quad (1)$$

where w_i is the weight control parameter. For this study, we ran with various weights $w_i \in \{1, 10, 100\}$ and found no appreciable difference between these settings. Hence, in the following, we use $w_i = 1$. As an aside, we note that this can be extended to multi-class classification in several ways, one

of them being adapting each w_i so that the effective number of samples of each class ($\frac{w_i}{n_i}$) is roughly the same (this can be done using the lowest common multiple of the number of samples in each class).

Clearly, as the minority class shrinks, there is a greater emphasis placed on getting these data points right. This is similar to oversampling by $\frac{w_i}{n}$ because with the unweighted loss function and oversampled data, an individual minority class data point (now represented multiple times) contributes more to the loss function than a majority class data point. However, using a weighted loss function has several advantages (a) we do not need to load additional samples into memory (b) we save computation since we do not need to run iterations over these samples (c) it is flexible: we can use a different weight easily.

We certainly are not the first to attempt a reweighted loss function: Lin et al. [67] propose a ‘‘focal loss’’ function that uses an exponential weighting scheme. However, to the best of our knowledge, we are the first to apply this with various weights in the software engineering domain.

2.3.4 Weighted Fuzzy Oversampling

We found experimentally that while using a weighted loss function does improve performance, we can do better still, through weighted fuzzy oversampling. This is motivated by a desire to achieve more ‘‘robust’’ decision boundaries. Let γ_i be the (shortest) distance from a minority sample to the decision boundary. Akin to the terminology used in support vector machines, we call γ_i the *functional margin*, and $\gamma = \min_i \gamma_i$ the *geometric margin*. Our proposed weighted fuzzy oversampling is motivated by the SVM goal to achieve $\hat{y} = w^T x + b \geq \gamma$. Using the same notation as the weighted loss functions, let n represent the fraction of samples in the minority class c_0 . Then, iteratively, we add the training samples $(\mathbf{x} - i\Delta\mathbf{r}, c_0)$ and $(\mathbf{x} + i\Delta\mathbf{r}, c_0)$ for each sample (\mathbf{x}, c_0) in the minority class. At iteration i , we add the new samples $\frac{(1/n)}{2^i}$ times; we iterate till this value is 0. Here, $\Delta\mathbf{r}$ (which we note is a vector) is a user-specified parameter; in our experiments, we found that values between 0.01 and 0.05 work well. We note here that it might be more beneficial to choose Δr based on the statistics of each attribute, but we do not explore that here. Algorithm 1 describes weighted fuzzy oversampling.

Algorithm 1: Weighted fuzzy oversampling

```

1 for each sample  $x$  in the minority class do
2   for  $i$  such that  $\frac{(1/n)}{2^i} \geq 1$  do
3     | Add  $(\mathbf{x} \pm i\Delta\mathbf{r}, c_0)$  to the training set;
4   end
5 end
```

2.3.5 PseudoCode

As shown in Algorithm 2, GHOST uses weighted loss functions (lines 10,24), DODGE’s ‘‘ ϵ -domiantion’’ (line 14), and weighted fuzzy oversampling (line 2). Having tuned the deep learner to (a) learn to recognize the minority class and (b) learn robust decision boundaries using the above methods, we found that GHOST achieved good recalls, but the learners now have trouble recognizing the negative examples, due to the minority class being *over-oversampled*. We

Algorithm 2: GHOST

```

1 Separate the data into train and test sets;
2 Apply weighted fuzzy oversampling to the training set;
3 Apply SMOTE to the resulting training set;
4 Choose a set of key hyperparameters and pre-processors;
5 Build a list of options for preprocessing and tuning, and assign
  every node a weight of 0;
6 Sample at random to create random combinations of
  preprocessors and hyperparameters;
7 for  $N_1$  random samples  $\theta_1, \dots, \theta_{N_1}$  do
8   for  $N_0$  epochs do
9     |  $\hat{y} = f(\mathbf{x}; \theta_i)$ ;
10    |  $\mathbf{w} = \mathbf{w} - \nabla_{\mathbf{w}} \hat{\mathcal{L}}(\mathbf{y}, \hat{y})$ ;
11   end
12    $\phi_i = \text{eval}(f)$ ;
13 end
14 for options that result in metrics with a difference  $< \epsilon$  do
15   | Reduce the weight by 1;
16 end
17 forall other options do
18   | Increase the weight by 1;
19 end
20 for  $N_2$  evaluations do
21   | Choose the options with the highest weight, and mutate its
     | parameter settings;
22   for  $N_0$  epochs do
23     |  $\hat{y} = f(\mathbf{x}; \theta_i)$ ;
24     |  $\mathbf{w} = \mathbf{w} - \nabla_{\mathbf{w}} \hat{\mathcal{L}}(\mathbf{y}, \hat{y})$ ;
25   end
26    $\theta^* = \text{argmax}_{\theta} \text{eval}(f)$ ;
27 end
28 return  $\theta^*$ 
```

fix this using SMOTE [68] with the default settings. SMOTE is a method for handling class imbalance. In the training set, this algorithm discards instances from the majority class while also generating artificial examples of the minority class (by extrapolating between near neighbors of the same class).

(Technical aside: SMOTE is different from weighted fuzzy oversampling– while SMOTE adds one point randomly between one of k nearest neighbors, weighted fuzzy oversampling radially oversamples points with an exponentially decaying weight.)

3 EXPERIMENTAL METHODS

The rest of this paper discusses a comparative evaluation of GHOST with DODGE, standard deep learning, and the results of Wang et al. [19], who use a Deep Belief Network [69].

3.1 Experimental Design

Our experiments compared different defect predictors. For non-DL learners, we used the methods from DODGE [12].

For DL learning, we adopted a deep learner with 4 layers and 20 units per layer, trained for 10 epochs (from Theorem 5 in Montufar et al. [37]) We choose this based on the results of their paper, which was published in a top machine learning conference (NeurIPS 2014). While some deep learning researchers might be surprised to see that we only trained for 10 epochs, we found experimentally that this was sufficient for AUC and popt20; for recall and false alarm rate, we used 30 epochs.

For deep belief networks (DBN), the methods seen in a TSE’18 paper by Wang et al. [19]. In that approach, foeach

file in the source code, they extract tokens, disregarding ones that do not affect the semantics of the code, such as variable names. These tokens are vectorized, and given unique numbers, forming a vector of integers for each source file. These vector-buggy pairs form the training set, which are input to a Deep Belief Network (DBN), a neural network architecture designed to extract features. The extracted features are used as input to a classical learner, such as Naive Bayes.

We use Wang et al. since (a) that paper made a convincing case that this approach represented a state-of-the-art results for defect prediction for features extracted from source code and (b) a deep learner is used to extract features, rather than being used as the learner, in contrast to our approach; this forms a valuable comparison.

We also explored two variants of defect prediction:

- *Within-project defect prediction (WPDP)*. Here, models learned from earlier releases of some project predict properties of latter releases of that project.
- *Cross-project defect prediction (CPDP)*. Here, models learned from project1 where then applied to project2.

To compare our results with other cross-project methods, we used TCA+ [70] since that paper reported that TCA+ performed as well, or better, than the prior work. We compare these against the results of GHOST. On experimenting with weights $w_i \in \{1, 10, 100\}$ (from Equation 1), we noticed no improvement over $w_i = 1$. Hence, we use that weight to optimize for each of the performance goals listed in §3.3.

When comparing against DODGE, in we split the data into train and test sets, as shown in Table 5. GHOST used the training sets to find good DL settings, which were then applied to the test suite.

We ran our experiments on two machines: (a) an Intel Broadwell CPU with 6 cores, 39GB RAM; and (b) a NVIDIA Tesla V100 GPU with 16 GB VRAM. The other had 15 GB RAM and a NVIDIA Tesla P100 GPU with 16 GB VRAM.

Because of the stochastic nature of deep learning (caused by random weight initialization), a statistically valid study of its merits should consider the distribution of its performance. For this reason, we run GHOST 20 times for each dataset, for each metric. All the results reported (in Table 6 and Table 10) are median values of these 20 runs.

3.2 Data

For this study, we use the same data used in DODGE’s prior study [12] on defect prediction: see Table 5. These are all open source Java projects from the PROMISE repository [71]. Each dataset has 20 static attributes for each software project, described in Table 4.

3.3 Performance Metrics

For this study, we use performance metrics widely used in the defect predication literature. Specifically, we evaluate the performance of our learners using three metrics: Area Under the ROC Curve (AUC) with false positive rate on the x-axis and true positive rate on the y-axis, recall, false alarm rate, and popt20. We use these measures to compare against the baseline of Agrawal et al. [12].

Recall measures the number of positive examples that were correctly classified by a learner:

$$\text{recall} = TP / (TP + FN)$$

where TP is the number of true positives and FN is the number of false negatives. The false alarm rate is the number of times a classifier incorrectly classifies an instance as a positive example, when it is in fact, not. Often, the false alarms observed in these experiments is very small. Specifically, in our experiments, many of the false alarm results were 0. Hence, we will use pf , defined as follows:

$$pf = FP / (FP + TN)$$

where FP is the number of false positives and TN is the number of true negatives.

The next metric, popt20 comments on the inspection effort required after a defect predictor is run. To calculate popt20, we list the defective modules before the non-defective modules, sorting both groups in ascending order of lines of code. Charting the percentage of defects that would be recalled if we traverse the code sorted in this manner in the y-axis, we report the value at the 20% point.

Finally, we report the area under the receiver operating characteristics (ROC) curve (AUC), with false positive rate on the x-axis and true positive rate on the y-axis.

Note that, with one exception, for most of these performance measures, *larger* values are *better* since:

- As recall increases, more defective modules are seen.
- As popt20 increases, the learner recommends that the developer inspect smaller parts of the code (to find bugs). That is to say, the learner is decreasing the work load of the developer.
- As AUC increases, our learners are finding more true positives and avoiding more true negatives.

The one exception is pf where *smaller* values are *better* since, as pf decreases, there are fewer cases where we waste a developer’s time showing them modules which are not really defective.

3.4 Impact of Performance Metrics

The original DODGE paper evaluated its models using metrics that were nuanced different to the above. For example, that study did not report recalls and false alarms. When we checked DODGE’s false alarms and recalls, we found that the results depended on the optimization goals; e.g. (a) optimizing for recall leads to higher recalls but nearly 100% false alarms; (b) optimizing for false alarms leads to near zero false alarms, but also near zero recalls. Hence, to ensure low false alarms and high, our optimizers guide their search by trying to maximize the harmonic mean of high recalls r and precision p ; i.e.

$$f_1 = 2rp / (r + p)$$

The results of that search were then assess via the performance measures listed in the last section.

To simplify DODGE’s search (and improve its performance scores), we applied a feature selection algorithm to the data sets prior to optimization. Feature selectors prune superfluous attributes (i.e. those not associated to

the number of defects). Following the advice of Hall and Holmes [72], we used Hall’s own CFS selector [73].

We also improved on the nine data pre-processors used by DODGE. To address issues of class imbalance then SMOTE tool [68] down samples the majority class while generating artificial examples of the minority class.

In summary, when we say “DODGE” below, we mean the original DODGE, optimizing for a specific metric, or maximizing F1 (in the case of recall and false alarm rate), after (a) feature selection with CFS and (b) SMOTE.

3.5 Statistics

The statistical methods used in this paper were selected according to the particulars of our experiments.

For example, in the *within-company experiments*, we are using learners that employ stochastic search. When testing such algorithms, it is standard [74] to repeat those runs 20 times with different random number seeds. Such experiments generate a distribution of 20 results per learner per data set. For those experiments, we use the *distribution statistics* of §3.5.1 to compare the efficacy of different learners.

For the cross-company experiments, we are comparing our results against the methods of Wang et al. [15], As mentioned in §2.2, we do not have access to their implementations but we do have access to the train and test sets they use. For these experiments, we must compare our results to the single performance points mentioned in the Wang et al. paper [15]. Hence, here we run train our learners once on their train data, then test once on their test data. Such experiments generated a single result per learner per data set. For those experiments, we use the *point statistics* of §3.5.2 to compare the efficacy of different learners.

3.5.1 Distribution statistics

Distribution statistics [56], [75] are used to distinguish two distributions of data. In our experimental setup, we run GHOST and DODGE 20 times, and therefore have a distribution of results for each dataset. This allows us to use distribution statistical methods to compare results.

Our comparison method of choice is the Scott-Knott test, which was endorsed at TSE’13 [76] and ICSE’15 [56]. The Scott-Knott test is a recursive bi-clustering algorithm that terminates when the difference between the two split groups is insignificant. Scott-Knott searches for split points that maximize the expected value of the difference between the means of the two resulting groups. Specifically, if a group l is split into groups m and n , Scott-Knott searches for the split point that maximizes

$$\mathbb{E}[\Delta] = \frac{|m|}{|l|} (\mathbb{E}[m] - \mathbb{E}[l])^2 + \frac{|n|}{|l|} (\mathbb{E}[n] - \mathbb{E}[l])^2$$

where $|m|$ represents the size of the group m .

The result of the Scott-Knott test is *ranks* assigned to each result set; higher the rank, better the result. Scott-Knott ranks two results the same if the difference between the distributions is insignificant.

3.5.2 Point statistics

For point statistics, we have access to various performance points (e.g. recall) across multiple data sets. To determine if one point is better than another, we have to define a delta Δ below which we declare two points are the same.

To that end, we use recommendations from Rosenthal [77] (as of June 2020, this paper has 2713 citations in Google Scholar). Rosenthal comments that for point statistics, parametric methods have more statistical power (to distinguish groups) than nonparametric methods. Further, within the parametric methods, there are two families of methods: those that use the “ r ” Pearson product moment correlation; and those that use some “ d ” normalized difference between the mean.

After making all those theoretical points Rosenthal goes on to remark that neither method is intrinsically better than another. Using Rosenthal’s advice, we apply the most straightforward method endorsed by that research. Specifically, we compare treatment performances differences using Cohen’s delta, which is computed as 30% of the standard deviation of all the values.

$$\Delta = 0.3 * \sigma \quad (2)$$

(Here, σ is the standard deviation of all the, e.g., recall measures seen across all the data sets in the cross-company experiments.) When two methods are different by less than Δ , we say that they *perform equivalently*. Otherwise, we say that one method *out-performs* the other if its performance is larger than Δ .

4 EXPERIMENTAL RESULTS

This rest of this paper uses the above methods to explore the research questions listed in the introduction.

4.1 Standard Deep Learning versus the Prior State-of-the-art (RQ1)

The results of this section address our **RQ1** which was **Does standard deep learning work for defect prediction?**

Table 6 compares DL versus the prior state-of-the-art (DODGE) versus our preferred new method (GHOST). **RQ1** comments on the DL-vs-DODGE comparisons (and other comparisons are discussed, later in this paper).

In the 40 experiments of Table 6, DL only performs better than or the same as DODGE in 10/40 experiments (see all the entries marked with “*”). A repeated patterns is that when DL wins on false alarm, it usually does so at the expense of low recalls. Also, when it does achieve a high recall (e.g. for velocity), this usually comes at the cost of high false alarms. Hence we say:

For defect prediction, standard deep learners do not perform better than existing state-of-the-art methods in 30/40 experiments.

Note that these **RQ1** results merely show that standard deep learning performs badly. Those results do not explain *why* that is so. For this task, we must move on to **RQ2**.

TABLE 6: RQ1 results. Cells show medians fo 20 runs. Cells with an "*" show the few cases where DL worked better than DODGE. **Dark blue** shows top rank (note: for pf, less is better), **light blue** shows rank two; white shows lowest rank (worst performance). Rankings were calculated via §3.5.1. The train and test versions used here are the same as the DODGE paper (see Table 5).

		AUC	popt20	recall	pf
ivy	DL	0.58	0.15	0.33	0.27 *
	DODGE	0.71	0.25	0.85	0.36
	GHOST	0.69	0.31	0.95	0.38
camel	DL	0.55	0.18	0.10	0.04 *
	DODGE	0.58	0.54	0.63	0.36
	GHOST	0.62	0.54	0.65	0.41
jedit	DL	0.61	0.10	0.46	0.16 *
	DODGE	0.63	0.39	0.64	0.25
	GHOST	0.68	0.41	0.91	0.40
log4j	DL	0.55	0.31	0.33	0.13 *
	DODGE	0.61	0.99	0.54	0.22
	GHOST	0.66	0.99	0.75	0.19
velocity	DL	0.50	0.57	0.90 *	0.87
	DODGE	0.61	0.64	0.76	0.47
	GHOST	0.68	0.64	0.82	0.47
synapse	DL	0.54	0.23	0.20	0.05 *
	DODGE	0.65	0.48	0.65	0.23
	GHOST	0.67	0.48	0.63	0.33
lucene	DL	0.58	0.51	0.69 *	0.52
	DODGE	0.61	0.80	0.67	0.36
	GHOST	0.59	0.80	0.7	0.34
xalan	DL	0.55	0.24	0.21	0.09 *
	DODGE	0.71	1.0	0.71	0.14
	GHOST	0.75	1.0	0.76	0.27
xerces	DL	0.52	0.28	0	0.04 *
	DODGE	0.59	0.93	0.54	0.15
	GHOST	0.62	0.94	0.57	0.39
poi	DL	0.61	0.36	0.45	0.18 *
	DODGE	0.72	0.66	0.78	0.22
	GHOST	0.73	0.74	0.78	0.38

TABLE 7: RQ3 results. GHOST vs. DODGE. Summary of Table 6. The first column indicates the number of wins, ties, and losses for each metric (these are defined using the Δ measure of Section 3.3 and the directions for "better" defined in Section 3.5. Note that for popt20 and pf, there are multiple ties because both DODGE and GHOST achieve the highest possible score.

	AUC	Recall	popt20	pf	Total
win	7	6	3	2	18
tie	1	3	6	1	11
loss	2	1	1	7	11
win + tie	8	9	9	3	29

TABLE 8: RQ3 results. GHOST vs. DL. Summary of Table 6. Same format as Table 7.

	AUC	Recall	popt20	pf	Total
win	9	9	10	2	30
tie	1	0	0	1	2
loss	0	1	0	7	8
win + tie	10	9	10	3	32

4.2 Why Does Standard Deep Learning Fail? (RQ2)

Our second research question was RQ2 which was **Why does standard deep learning fail for defect prediction?** Recall from Table 5 that the class balance between defective and non-defective modules is highly variable between our train and test sets. We conjecture that DL’s failure to defeat tradition methods was that these algorithms were not tuned for such highly variable class balances. Preliminary results with an artificially generated data set (recall Figures 3a and 3b) showed that unweighted loss functions means that a neural net can miss the minority class. Those artificial results make us conjecture that:

The lack of success of deep learning in defect prediction can be attributed to optimizing for the wrong performance metric.

This conjecture is tested in the next research question.

4.3 How to Fix Deep Learning? (RQ3)

RQ1 assessed DODGE with respect to standard DL. This checks if GHOST offers any added value over DODGE.

Returning to Table 6, GHOST can be seen to have more **dark blue** cells than anything else (i.e. statistically, it is ranked number one most often). ty), it also has a high false alarm rate. Table 7 and Table 8 summarize those results for comparison with DODGE:

- 18 times, GHOST defeats DODGE (see top-right, Table 7);
- 29 times, GHOST defeats DL (see top-right, Table 8);
- 11 times, GHOST was as good as DODGE (see mid-right, Table 7)

TABLE 9: Comparison of GHOST (optimized for recall) with the results of Wang et al. [15], using deep learning-generated "Semantic" features to train a Naive Bayes model on within-project defect prediction. Bold indicates better results, as determined by the point statistics of §3.5.2 (and here, $0.3\sigma = 4.91$).

Dataset	Train	Test	Wang et al.			GHOST		
			P	R	F1	P	R	F1
synapse	1	1.1	46	66.7	54.4	44.1	100	50.4
	1.1	1.2	57.3	59.3	58.3	44.1	100	56.1
jEdit	3.2	4	46.7	74.7	57.4	42.3	100	55.9
	4	4.1	54.4	70.9	61.5	40.5	100	54.8
log4j	1	1.1	67.5	73	70.1	55.6	100	65.9
ivy	1.4	2	21.7	90	35	24.9	100	37.8
lucene	2	2.2	75.9	56.9	65.1	61.3	100	74.5
	2.2	2.4	66.5	92.1	77.3	60.9	100	75.3
camel	1.2	1.4	96	66.4	78.5	20.2	100	32.4
	1.4	1.6	26.3	64.9	37.4	26.7	100	38.2
xalan	2.4	2.5	65	54.8	59.5	62.7	100	66
xerces	1.2	1.3	40.3	42	41.1	18.6	100	30
poi	1.5	2.5	76.1	55.2	64	72.2	100	83.2
	2.5	3	81.6	79	80.3	70.2	100	79.7
ant	1.5	1.6	88	95.1	91.4	65.5	84.3	62.8
	1.6	1.7	98.8	90.1	94.2	52.6	99.4	57.3

Tables 6, 7, 8 display distribution results where algorithms were run multiple times using different random number seeds. Tables 9 and 10, on the other hand, show display point distribution results where our algorithms are compared to the single set of performance points reported in prior work.

This is also seen in comparison with the results of Wang et al., where we perform as good or better in 11/16 datasets for within-project defect prediction. In cross-project defect prediction, we also note that we win 9/20 times and tie 5/20 times; this suggests that GHOST may be able to generalize well across different projects.

Table 11 summarizes the comparison with the results of Wang et al. [15].

- For within-project defect prediction (WPDP), GHOST (tuned for recall) defeats a Deep Belief Network (DBN) 3 times, ties 2 times, and loses 5 times. This suggests that tuning for recall does not necessarily lead to better F-1 scores (but does lead to better recalls).
- For cross-project defect prediction (CPDP), GHOST (tuned for F-1 score) defeats DBN-CP 9 times, ties 5 times, and loses 6 times.
- We note here that GHOST performs far better for cross-project defect prediction (9/14 = 64.3% wins vs. 3/11 = 27.3% wins). This could be indicative of strong generalizing ability of GHOST.

TABLE 10: Comparison of GHOST with the results of Wang et al. [15] on cross-project defect prediction. All results shown are F-1 scores. GHOST results are medians over 20 runs. Bold and dark blue indicates better (in case of tie, both are in bold), as determined by point statistics of §3.5.2 (and here, $0.3\sigma = 4.41$).

Source	Target	Wang et al.	GHOST
jedit-4.1	camel-1.4	69.3	34.9
camel-1.4	jedit-4.1	61.5	47.3
lucene-2.2	log4j-1.1	61.8	51.7
xerces-1.3	ivy-2.0	45.3	28.3
synapse-1.2	ivy-2.0	82.4	30.3
camel-1.4	ant-1.6	97.9	62.7
xalan-2.5	xerces-1.3	38.6	35.7
ivy-2.0	xerces-1.3	42.6	40.7
log4j-1.1	jedit-4.1	50.3	49.4
jedit-4.1	log4j-1.1	64.5	64.1
lucene-2.2	xalan-2.5	55	65.4
xerces-1.3	xalan-2.5	57.2	66
xalan-2.5	lucene-2.2	59.4	73.8
log4j-1.1	lucene-2.2	69.2	74.6
ivy-2.0	synapse-1.2	43.3	60.9
poi-3.0	synapse-1.2	51.4	58.1
synapse-1.2	poi-3.0	66.1	82.7
ant-1.6	camel-1.4	31.6	36
ant-1.6	poi-3.0	61.9	77.8
poi-3.0	ant-1.6	47.8	62.5

TABLE 11: Summary of GHOST vs Wang et al. [15]

	WPDP (Tbl. 9)	CPDP (Tbl. 10)	Total
win	3	9	12
tie	8	5	13
loss	5	6	11
win + tie	11	14	25

These results recommend GHOST over DODGE and DBN. Also, they deprecate the use of off-the-shelf standard DL for defect prediction (since GHOST clearly is preferred to standard DL). We attribute the super performance of GHOST to its weighted loss function.

The exceptions to the above pattern are the GHOST vs DODGE recall results, which we will discuss in the next section. Apart from that, we say that:

For most evaluation goals, this modified version of deep learning performs better than the prior state-of-the-art.

4.4 Use Deep Learning in All Cases? (RQ4)

The results of this section address our RQ4 which was **Does deep learning work well in all cases?**

The false alarm rate results for GHOST in Table 6, while respectable, loses to DODGE. Note that we made considerable efforts to improve the results (e.g. weighted fuzzy oversampling, SMOTE, optimizing for different metrics), to no avail. Hence we offer the following:

- In domains where the goal is to “cut corners” to find the most defects with the least effort, then we want to optimize for popt20 (since that criteria leads us to models that find most bugs with least inspection effort). In those domains, the results of Table 7 recommend DL with GHOST
- Our results also lend support to using GHOST in systems where raising false alarms is less disastrous than missing a true positive (recall).
- In other domains where it is mission critical to raise few false alarms, then the Table 7 show us that DODGE than GHOST (or DL) for better predicting recall.

Hence we say:

We recommend the use of deep learning in domains where false alarms are not particularly catastrophic.

4.5 Scalability of Deep Learning (RQ5)

The results of this section address RQ5 which was **How slow is tuning for deep learning for defect prediction?**

To answer this question, we report the median training time over 20 runs. The measured time is CPU time, when trained on a 4-core Intel Core i5 CPU. These are summarized in Table 12. Clearly, our models are very fast (less than 2 seconds to train on a CPU).

TABLE 12: Median training time over 20 repeats.

	Training (secs)	GHOST (secs)
ivy	0.78	9m 17s
lucene	0.81	10m 26s
poi	1.09	13m 44s
synapse	0.81	9m 48s
velocity	0.88	10m 20s
camel	1.41	18m 10s
jEdit	1.11	14m 32s
log4j	0.73	8m 29s
xalan	1.61	20m 16s
xerces	1.08	11m 56s

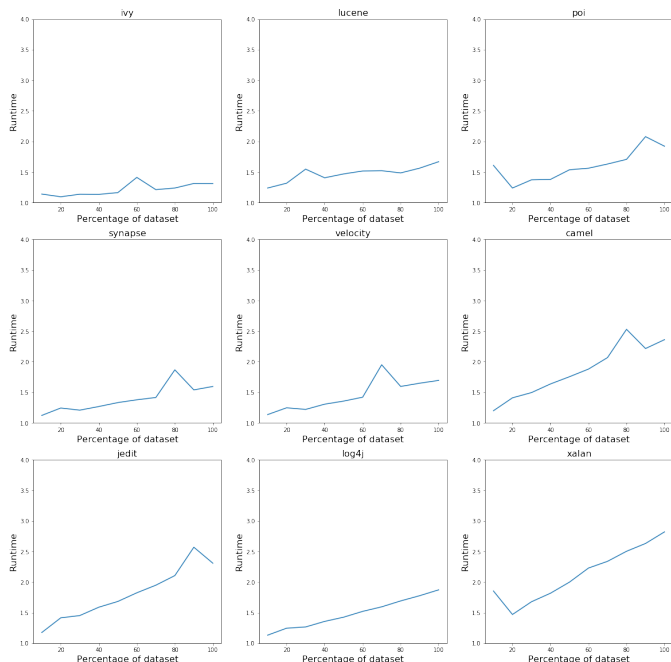


Fig. 4: Scalability results for 9 datasets. The x-axis is the percentage of the dataset trained on; the y-axis is the runtime in seconds on a CPU.

Table 12 also shows the runtimes for running GHOST on different datasets. Because GHOST runs 20 times to find the median value of a metric, the runtimes we report are for all 20 runs. However, we do not divide this time by 20 as we feel it is scientifically important to run a stochastic experiment multiple times and report statistical results.

Figure 4 shows our scalability results, which we obtain by measuring the training time for different sizes of the datasets. We observe a general trend across all datasets that deep learning scales well with the size of the problem. More specifically, GHOST’s runtimes grows sublinearly. For example, in the xalan results:

- A 500% increase in data (from a fifth to all the data)...
- ... leads to a runtime increase of only $2.7/1.5 = 180\%$.

Hence we say:

Tuning deep learners is both practical and tractable for defect prediction.

5 THREATS TO VALIDITY

Sampling bias: As with any other data mining paper, it is important to discuss sampling bias. We claim that this is mitigated by testing on 10 SE projects over multiple versions, and demonstrating our results across all of them. Nevertheless, in future work, it would be useful to explore more data.

Learner bias: Our learner bias here corresponds to the choice of architectures we used in our deep learners. As discussed above, we chose the architectures based on our reading of “standard DL” from the literature. That said, different DL architectures could lead to different results.

Evaluation bias: We compared our methods using a range of metrics: AUC, recall, false alarm rate, and popt20. We used these varied evaluation metrics to demonstrate more clearly the benefits of not using learners off-the-shelf, and tuning the loss function optimized. If other performance metrics are used, then other results might be obtained.

Order bias: This refers to bias in the order in which data elements appear in the training and testing sets. We purposely choose these so that the testing set is from a newer version than the data used in the training set, following the natural order of software releases. Therefore, we argue that such a bias is needed to mimic real-world usage of such learners.

External validity: We tune major hyperparameters using DODGE, removing external biases from the approach. Our baseline results are based on the results of Montufar et al. [37], which has been evaluated by the deep learning community.

6 CONCLUSION

This paper has argued:

For SE, do not use off-the-shelf deep learning. Instead, tune that algorithm to the needs of SE (using tools like, e.g. GHOST).

To make that case, this paper explored GHOST, a new method that combines deep learning and the ϵ -domination heuristic with a set of tweaks: (a) weighted loss functions (see §2.3.3) (b) weighted fuzzy oversampling (see §2.3.4) (c) SMOTE [68], an oversampling method. We demonstrated the efficacy of our approach on 10 defect prediction datasets, using four metrics. We compared our results against two sets of baselines, one being a reasonable baseline based off deep learning research and prior state-of-the-art result in defect prediction, and the other being a prior result using deep learning to extract features from code. We presented scalability and runtime tests to demonstrate that deep learners can be trained quickly. We showed that best results come from learners properly tuned for the dataset and the goal metric (using a weighted loss function).

Finally, we note that one could apply the weighted loss functions to classical learners, such as support vector machines (SVMs), which rely on an optimization setup.

We take care to stress that our results on relate to defect prediction. As to other areas of software analytics, that is a matter for future search. That said, our results suggest that, for future work, the following research agenda could be insightful:

- 1) Divided analytics into various domains.
- 2) Then, for each domain:
 - a) Determine the prior non-DL state-of-the-art;
 - b) Compare DL with that state-of-the-art;
 - c) Seek ways to better adapt DL to that domain.

ACKNOWLEDGEMENTS

This work was partially funded by a research grant from the National Science Foundation (CCF #1703487).

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